

LETTERS TO THE EDITOR.

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The Positive Charge carried by the α Particle.

SOME time ago I made the suggestion in NATURE (March 9, 1905) that the α particle was initially uncharged on expulsion, and that it gained its charge subsequently by collision with atoms in its path. I need only now repeat that the suggestion was based on the brilliant work of Bragg in Australia, who showed that the α particle passes through, rather than collides with, the atoms of solid or gaseous matter in its path, and that whether uncharged or not initially, it must, equally with the atom struck, become charged positively after the encounter by the detachment of a negative electron.

Recently P. Ewers (*Physikalische Zeitschrift*, March 1), using the α particles from polonium, attempted to put the view to an experimental test with negative results, and concluded against the probability of the hypothesis. Bragg (*Phys. Zeit.*, July 1) has pointed out that Ewers's experiments by no means settle the question, and, indeed, he evidently considers it a question which cannot be settled experimentally. Certainly the requisite conditions to be fulfilled for a positive result are so rigorous that no one could be certain they had been fulfilled, and it is impossible to disprove the view by a negative result. But it is obvious that a positive result, that is, the actual isolation of the α particle in an uncharged state, would settle the question. This I have been fortunate enough to do, although only after a long experience of negative results where it might reasonably have been concluded the requisite conditions had been realised. "The best laid schemes of mice and men gang aft a-gley." A determining factor in the problem conditioning whether a positive or negative result is obtained could not possibly have been foreseen, and it was only when all hope of getting anything but a negative result had been abandoned, and what was intended to be a final experiment was being performed, that a slight change in one of the factors happened to eliminate the disturbing cause, and I obtained the coveted positive result. The precise nature of this disturbing influence is, perhaps, not yet fully demonstrated, although personally I think I now hold the clue. But there is not the slightest doubt that the α particle initially expelled is not charged as the experiments given prove.

The essential conditions are two. In the first place, the α particle must be examined in a vacuum such that during its path it does not encounter a single gas molecule. Secondly, the layer of radio-active matter from which it is expelled must not be more than one molecule thick, and must not be mixed with or overlaid by inactive matter. These conditions being fulfilled, the α particle will not traverse a single atom after expulsion, and if uncharged initially must remain so. As a third condition, it is desirable that the test for the charge shall be made on the particle during its flight. It is at least conceivable that an uncharged α particle striking a plate will convey to it a positive charge if the electron detached from the uncharged α particle on impact has sufficient energy to escape the plate.

The second condition is, as may be imagined, the difficult one to make sure of. I hoped to secure it by using radium C as the source of the rays. The rate of its disintegration is so rapid that there is only just the necessary time for an experiment to be carried out. Hence the actual number of atoms of the radio-active substance is for radium C the minimum it is possible to employ. Moreover, this number can readily be calculated, and since it is deposited from a gas uniformly on the exposed surface, not only can an experiment be devised so that the thickness of the deposited layer fulfils the monomolecular condition, but, what is equally important, it can be assumed with reasonable certainty that the radio-active layer is not overlaid or mixed with inactive matter.

With regard to the first condition, all the factors are known, and the necessary conditions can readily be calculated by two independent methods, which, as it proved, are strikingly verified by the actual results obtained. The only pitfall is in the altogether exaggerated impression which is abroad as to the ease with which a high degree of vacuum can be obtained by modern methods.

The third condition was realised by using the magnetic deviation of the α rays as a test for their charge. The rays passed out of the capillary tube from a deposit of radium C at the far end. This was obtained by the use of the emanation from 30 mg. of radium. Conditions were arranged so that the rays were completely deviated under ordinary conditions, and with the magnetic field on did not succeed in escaping from the tube, and the experiment consisted simply in re-examining the deviation in the highest vacuum that could be produced.

Long series of negative results led to the refinement of each essential condition until it seemed no further improvement was possible, and a wide margin of probability that the essential conditions had been realised had been secured. A most unmistakable negative result was obtained. But the next experiment intended to confirm this finally was as unmistakable a positive result as the other had been a negative one. In a partial vacuum the rays were completely deviated. In the highest vacuum the field made no perceptible difference. Between the two experiments there were two slight differences of conditions: (1) In the second experiment the radio-active deposit had been heated *in vacuo* after removal of the emanation and disappearance of radium A in order to remove a possible overlying film of condensed gas. (2) In the first experiment the emanation had been left in the capillary 2 hours 25 minutes, in the second 1 hour 30 minutes, the volume occupied by the emanation being less in the latter case.

In a third experiment the heating of the radio-active surface was omitted, and the emanation was allowed to act for only 45 minutes. The result was unequivocally positive.

In a fourth experiment the film was heated, and the emanation left in 1 hour 20 minutes, reproducing practically the conditions of the second experiment. Again the result was positive, and the magnetic field produced no appreciable effect in a high vacuum. But this experiment was continued for nearly two hours after the start, and at the end of the time the radiation, although, of course, much enfeebled, was quite intense enough for the purpose. As time elapsed a change came over the experiment. Little by little, the rays began to be affected by the field. This change was hastened by heating the active film in place in the high vacuum. At the end the result was as unequivocally negative, all the rays being deviated by the field in the highest vacua, as at the start it had been positive.

The clue, I think, is the change of the glass surface of the capillary, which it experiences under the excessive bombardment to which it is exposed, and which is indicated by the blackening of the glass. In the lead glass used it was remarked independently that the darkening appeared to commence somewhat suddenly. At the conclusion of the experiment it was always marked. But on cutting down the capillary before the commencement in the three final experiments with relatively short exposure to the emanation the darkening had not commenced, whereas in the last experiment, when the pole pieces were removed to allow the deposit to be heated in place it was noted that the darkening had begun. It can be imagined that the slightest roughening of the surface is all that is necessary to cause a negative result. The whole series of experiments from start to finish is explained if accompanying the darkening of the glass there is also a slight roughening. Whether this will prove sufficient to be within the range of the microscope remains to be seen.

I hope to examine the hypothesis that the blackening of the glass is accompanied by the roughening of the surface more in detail later. But whether this or some other explanation proves correct there can, I think, be no doubt about the conclusion that the α particle has been isolated under conditions in which it is not deviated by a magnetic field, and, therefore, is not charged. The theoretical consequences of the discovery need not here be dealt with. Cer-

tainly it looks as if the influence of electricity in radioactive change, and its importance generally in its relation to matter, could be overestimated.

FREDERICK SODDY.

The University, Glasgow, July 29.

Stress in Magnetised Iron.

THE important question whether there is any mechanical stress in an iron rod or ring when magnetised, and, if so, whether the stress is compressive or tensile, was discussed in NATURE ten years ago (vol. liii., pp. 269, 316, 365, 462, 533), but has not yet, so far as I know, received any generally accepted answer. That a magnetised rod must necessarily be in the same condition as if under a mechanically applied compressive stress tending to shorten the iron, was, I believe, first suggested by myself (Phil. Trans., vol. clxxix., p. 216, 1888). Those who support this view generally speak of the stress as "Maxwell's stress," and assume its value to be $B^2/8\pi$. The stress in question seems, however, to be quite unconnected with the "stress in the medium" proposed by Maxwell, and its value is not in general exactly $B^2/8\pi$, but $(B^2 - H^2)/8\pi$. I have lately had occasion to consider the problem again, and perhaps I may be allowed to re-state my argument in a slightly altered form, and illustrate it by means of an imaginary model.

If a uniformly magnetised rod is divided transversely, and the cut faces are brought close together, the magnetic force inside the narrow gap will be $B = H + 4\pi I$. The force acting on the magnetism of one of the faces, and urging this face towards the other, will be less than B by $2\pi I$, the part of the total force due to the first face itself; hence the force per unit of area with which the faces would press against each other if in contact is $P = (B - 2\pi I)I = 2\pi I^2 + HI = (B^2 - H^2)/8\pi$. (In the case of an endless permanent magnet, $H = 0$, and $P = B^2/8\pi$.) The width of the gap may be diminished until it is no greater than the distance between two neighbouring molecules, when it will cease to be distinguishable; but, assuming the molecular theory of magnetism to be true, the above statement will still hold good for the intermolecular gap. The same pressure P will be exerted across any imaginary section of a magnetised rod, the stress being sustained by the intermolecular springs, whatever their physical nature may be, to which the elasticity of the metal is due. The whole of the rod, therefore, will be subject to a compressive longitudinal stress P , the resulting contraction, expressed as a fraction of the original length, being P/M , where M is Young's modulus for the metal.

Let a magnetic molecule of iron be represented by a rigid steel sphere, uniformly magnetised and covered with a closely fitting shell of india-rubber, to play the part of the "intermolecular springs." Imagine a straight row of these spheres in contact with one another, and kept in place by a force analogous to cohesion, which, while binding the spheres together, leaves them free to turn on their centres. This arrangement would, for present purposes, serve as a model of a filament of iron one molecule in diameter. If the magnetic axes of the spheres pointed indifferently in all directions, the attractions would be balanced by the repulsions, and the length of the filament would be the same as if the spheres were unmagnetised. If, however, the magnetic axis of every sphere pointed in the same direction along the filament, as would be the case when the filament was magnetised, the india-rubber between all the pairs of unlike poles would be compressed and the filament would be shortened. Let F be the compressive stress across the rubber between a single pair of poles, and s the amount, expressed as a fraction of a centimetre, by which the rubber is contracted; then, if there are n spheres, the total contraction will be ns (n being assumed so great that it is sensibly equal to $n \pm 1$), which is the same as would be caused by an equal compressive stress F applied at the two ends of the unmagnetised filament. The whole filament when magnetised may therefore be regarded as under compressive stress due to the magnetic forces, and since Young's modulus $M = Fl/ns$, where l is the length of the unmagnetised filament, the contraction expressed as a fraction of the length is, as

originally stated, F/M , the value of F in an actual piece of iron being $2\pi I^2 + HI$.

Sometimes there may presumably also be a longitudinal tension, as in the case of an iron rod placed along the lines of force in a uniform field, when the tension would be HI . In a ring electromagnet this would not exist.

As to what effect would be produced in magnetised iron by Maxwell's distribution of stress in the ether, I cannot venture an opinion. But if there is a tension, it can hardly have the familiar value $B^2/8\pi$, which is possible only when B is equal to H , and there is no magnetisation ("Electricity and Magnetism," § 643). My point is that an important component of the stress in magnetised iron is a compression which can be calculated and allowed for. The question whether or not this view is tenable is of the highest interest in connection with the possible correlation of magnetic phenomena, and urgently needs an answer.

SHELFORD BIDWELL.

The Mixed Transformation of Lagrange's Equations.

I SHOULD fancy from the review by "G. H. B." in NATURE of July 19 (p. 265) that the papers of Prof. Levi Civita relate largely to the *mixed* transformation of Lagrange's equations, the complete theory (Proc. Camb. Phil. Soc., vol. vi., p. 117; "Hydrodynamics," vol. i., p. 171) of which was first given by myself so far back as 1887. But what I wish to point out is this, that this theory depends no more on any so-called theory of "ignored" coordinates (or *kinosthenic* coordinates as Prof. J. J. Thomson [Phil. Trans., 1885, part ii.] calls them) than it does on the existence of the hypothetical personage known as the Man in the Moon.

The theory is merely the result of a piece of elimination, and is as follows:—Let the coordinates of a dynamical system be divided into two groups θ and χ ; let Θ and κ be the momenta of types θ and χ ; and let T be the Lagrangean expression for the kinetic energy. Then it can be shown that

$$T = \mathcal{T} + \mathfrak{K} \dots \dots \dots (1)$$

$$\frac{\partial T}{\partial \dot{\chi}} = \kappa \dots \dots \dots (2)$$

$$\frac{\partial T}{\partial \dot{\theta}} = \Theta = \frac{\partial \mathcal{T}}{\partial \dot{\theta}} + \bar{\Theta} \dots \dots \dots (3)$$

where \mathcal{T} is a homogeneous quadratic function of the velocities $\dot{\theta}$, \mathfrak{K} is a similar function of the momenta κ , and $\bar{\Theta}$ is a linear function of the κ 's.

By means of (2) all the velocities and accelerations of type χ can be eliminated from Lagrange's equations, and the result is expressed by means of the modified Lagrangean function

$$L = \mathcal{L} + \Sigma(\bar{\Theta}\dot{\theta}) - \mathfrak{K} - V \dots \dots \dots (5)$$

and

$$\dot{\chi} = \frac{\partial \mathfrak{K}}{\partial \kappa} - \Sigma\left(\dot{\theta} \frac{\partial \bar{\Theta}}{\partial \kappa}\right) \dots \dots \dots (6)$$

Equations (5) and (6) constitute the mixed transformation of Lagrange's equations, and include the equations of Hamilton as well as those of Lagrange.

When the coordinates χ are *kinosthenic* coordinates, that is to say, coordinates which enter into expression for the energy of the system only through their differential coefficients with respect to the time, all the κ 's are constants, and (5) is sufficient to determine the motion.

In § 173 of my "Hydrodynamics," the words "the latter of which does not enter into the expression for the energy of the system" should be omitted.

A. B. BASSET.

Two Modifications of the Quartz Wedge.

SOME little time ago I wished to make a quartz wedge for producing interference colours with the polarising microscope. The usual wedge supplied by optical instrument makers seldom gives colours lower than "clearer gray" of Newton's colour-scale according to Quincke, while the lower colours are often particularly valuable in petrological work. The quartz wedge is described in the